This manuscript by Samin et al. presents two new $\delta^{18}O$ records from firn cores at Little Dome C, East Antarctica, and uses an updated VFC model to investigate the effects of diffusion, surface mixing, stratigraphic noise, and storage diffusion at different depth scales. The study shows that a classical diffusion-only model cannot reproduce the observed $\delta^{18}O$ variability, highlighting the importance of surface mixing in the near-surface layer and the influence of storage diffusion in one of the cores. The authors also argue that firn ventilation is more relevant at cold Antarctic sites than in Greenland.

The topic is highly relevant to the ice-core community, and the manuscript presents a substantial amount of data and modeling work. The overall reasoning is coherent, and the main findings—particularly the depth-scale paradox, the role of surface mixing, and the effect of storage diffusion—are convincing. However, a number of minor revisions and clarifications are needed to improve clarity and completeness. The manuscript is suitable for publication after these revisions.

Major comments:

1. The abstract would benefit from further revision to improve the logical flow, as the connections between different parts currently feel somewhat disjointed. In particular, too much emphasis is placed on the objectives and significance of the study, while the description of the results remains relatively brief. Additionally, although some key conclusions are mentioned, several valuable findings—such as the influence of storage processes on ice core isotopes, as well as the potential effects of surface mixing and stratigraphic noise—do not appear to be adequately reflected. A more balanced presentation of results and conclusions would strengthen the abstract.
2. The two ice cores were processed using different sample segmentation methods (one was directly segmented, while the other was analyzed by CFA continuous flow analysis). Could this difference introduce measurement errors for water stable isotopic profile? How did the authors consider the potential impact of such methodological differences on the model evaluation?
3. The description of the VFC model lacks clarity regarding how the different post-depositional processes are specifically coupled—for example, whether these processes are linked through a particular parameter. The description remains too general to allow a thorough evaluation of the model's operational mechanism, which in turn compromises the completeness and reproducibility of the methodological workflow. This is especially important given that the authors claim to have improved upon the original VFC model by incorporating surface mixing and the stratigraphic noise. Could the authors provide a more detailed explanation in this section, or perhaps include a detailed explanation in the appendix to ensure completeness and reproducibility?
4. Some of the model parameters used by the authors are derived from Dome C rather than

from the LDC site. This choice may introduce some bias, and the authors should provide a justification for this in the discussion section.

5. When generating synthetic monthly data of precipitation $\delta^{18}O$, the origin and applicability of the temperature-isotope conversion coefficient are questionable. The gradient of $0.5‰ \cdot °C^{-1}$ may only be derived from the results at Dome C over the past decade. Whether this coefficient is applicable to the LDC region and to the past two thousand years remains unclear and should be addressed.
6. The identification of sulfate peaks should be described in more detail. The identification of significant peaks would normally require an iterative method, but in this study, the description only states that sulfate peaks were determined based on Dome C results, without providing a detailed explanation. Additionally, differences in sulfate peaks between the two ice cores are observed, but no detailed explanation is given. A more thorough description would improve the reliability of the dating results.
7. The calculation of the RMS score is based only on the differences in PSD at two specific frequency points (the inflection point A and the high-frequency upper limit point B), rather than on the entire spectrum or the whole depth profile. This approach provides very limited information and may not adequately reflect how well the model matches the observations across the full frequency spectrum or the entire depth range. Additionally, is it reasonable to use average weighting in this calculation? Could the authors provide a detailed explanation of the considerations behind this formula?
8. The authors explain that even when incorporating storage effects, the influence on ice core isotopes still cannot effectively reconcile the differences between the VFC model simulations and the Subglacior-LDC observations in terms of power spectral density. They suggest that adding storage effects would improve the agreement. However, as shown in Figure 13, at high frequencies, the VFC model simulations already fall below the actual observations, while at low frequencies, the simulations remain higher than the observations. This pattern suggests that simply increasing the storage effects may not necessarily lead to a better match between the model and the observations. Other factors might be at play. In this regard, the authors should provide further explanation and clarification.
9. The conclusions might be better restricted to the LDC site rather than being generalized to broader contexts. Limiting the scope of the conclusions to the specific study site would help avoid overextrapolation.

Minor comments:

1. The writing in the main text and the abstract is sometimes too casual, and some sentences are hard to follow, especially in the Discussion. To improve overall clarity and readability, it might be helpful to have the manuscript carefully checked and revised by a native English speaker.

2. Multiple references are separated by commas; these should be changed to semicolons.
3. Section 2.1: Please state the depths of the two ice cores in this section
4. Section 2.2, second paragraph: The tense is inconsistent. Please revise.
5. Section 2.3: The description of the sulfate measurement method using the Subglacior-LDC is missing. Please provide the method and a reference citation.
6. Section 2.3: Can the dating accuracy or error be provided?
7. Section 2.5, Line 183: Does the calculation of diffusion length require the atmospheric pressure parameter? Please clarify.
8. Appendix B: The content is too brief, only showing results. Appropriate text and necessary formulas should be provided to explain the calculation process.
9. Section 3.1: After removing the top 3 m, the standard deviations of the isotope data in both two cores become larger. This is unreasonable because diffusion should reduce high-frequency isotopic fluctuations with increasing depth (as stated in the introduction). Such an effect will decrease the standard deviation. So, please check the accuracy of the data and provide a reasonable explanation for this result.
10. Table 1: Units are missing, and the necessity of this table is not clear. Please revise or justify.
11. Figure 3: The y-axis mark in the second subplot is incomplete, and the label for sulfate lacks the ion symbol (SO_4^{2-}).
12. Section 4.2: The authors state that a 6–8 cm mixed layer is "consistent with Ollivier et al." However, Ollivier et al. reportedly proposed a 4 cm mixed layer. Unless Ollivier et al. also considered 6 – 8 cm as possible or within their uncertainty range, this is a logical contradiction. It seems the intended meaning might be that the 6–8 cm mixed layer is consistent with Ollivier et al.'s interpretation that the mixed layer is driven by wind and snow properties, rather than consistent with the numerical value of 4 cm. However, the current wording is misleading. Please clarify explicitly what "consistent" refers to.
13. Numerous spelling errors and inappropriate word usages are found throughout the text. The following list provides only some of the visible problems, but these are by no means exhaustive. The authors should carefully check and revise all language issues in the manuscript.

Line 16: "effect" → "process"

Line 17: "water isotopes profiles" → "water isotope profiles"

Line 21: "a 8 cm" → "an 8 cm"

Line 28: "allowing" → "which allows"

Line 46: "keeps" → "preserves"

Line 62: "vapour" → "vapor"

Line 75: "remains" → "is"

Line 79: "deposition" → "depositional"

Line 104: "sulphates" → "sulphate"

Line 111: "samples of 2.5 cm" → "samples of 2.5 cm length"

Line 120: "peristatic pumps" → "peristaltic pumps"

Line 124: "checked" → "verified"

Line 125: "by" → "with"

Line 127: "to" → "and"

Line 129: Please delete "established at".

Line 138: "the three last minutes" → "the last three minutes"

Line 151: "30 mMol" → "30 mM"

Line 158: "in" → "by"

Line 165: "disturb" → "disturbs"

Line 190: "on" → "to"

Line 218: "et" → "and"

Line 241: There is a minus sign before the numerical value (-50.89‰). Please check if this is correct.

Line 252: "depth" can be deleted.

Line 263: Please add "where" at the beginning of this sentence.

Line 276: "cores" → "core"

Line 292: "3-meters" → "3 meters"

Line 393: "previoulsy" → "previously"

Line 404: "test" → "tests"

Line 432: "suggested" → "suggests"

Line 449: "i.e." → "i.e.," (add comma)

Line 482: "initially" → "initially"